corrugated and torn spore-membrane. I have not succeeded in discovering any purpose whatever in it, striking as the appearance is.

All these observations on the formation of spores confirm the *general results* which H. von Mohl laid down in his memoir on the development of the spores of *Anthoceros lævis,* Linnæa, 1839, vol. xiii. p. 273—290.

1. *Four spores are always developed in a mother-cell.*

2. *Previously to their development, a granular fluid matter is contained in the mother-cell.* Here it may be added, that this same is formed of the dissolved cytoblasts.

3. *The four spores are formed at the same time, and certainly not,* as Mirbel believed, *by the mechanical division of the cell-mass into four parts by septa, these septa proceeding from the membrane of the mother-cell, but in an independent manner.* To this it may be added, that actual cytoblasts are simultaneously produced in the cell.

The chief conclusion therefore is, *that the process of spore-formation does not differ from the formation of cells through cytoblasts. Psilotum* cannot be too highly recommended for the observation of all these facts, as we here possess extraordinarily large mother-cells which allow all the alterations in their interior to be perceived with the greatest distinctness.

Diversity in the peculiarities of the formation of spores in *Psilotum* from that in *Anthoceros* and other Cryptogamic plants, is of course owing to family and generic differences.

[To be continued.]

XXXI.—*On the Siliceous Bodies of the Chalk and other Formations, in reply to* Mr. J. Toulmin Smith. By J. S. Bowerbank, F.R.S. &c.

In the last January Number of the 'Annals and Magazine of Natural History' there are some observations by Mr. J. Toulmin Smith on the Formation of the Flints of the Upper Chalk, in which the author combats certain conclusions of mine published in the 'Transactions of the Geological Society,' vol. vi. new series, p. 181, relative to the spongeous origin of the flinty bodies of the chalk, greensands, and oolites. Had the differences between the author and myself been merely matters of opinion, I should not have occupied your valuable pages on the present occasion, especially as he has declared* that "it is not his intention to dispute the particular facts stated by myself as applying to the cases

* Page 2.

I have observed;" and granting this, I have really very little more to desire, as the whole of the views exhibited in my paper are attempts to elucidate obscure natural phænomena and not matters of theory; but as in the course of his reasoning upon the evidence which I have produced of the organic origin of the siliceous bodies in dispute, he has referred to natural historical facts in support of his views, and ventured upon assertions based upon these facts which are unfortunately not correct, I should not be doing justice either to the subject or myself if I were not to endeavour to correct these misapprehensions.

The author, after noticing the difference in the views of the formation of chalk flints entertained by Prof. Ehrenberg and myself, proceeds thus*: "It may be allowed to us, in all humility, to call in the aid of other classes of facts to clear up the mystery, and this I now proceed to do. I fully admit that spiculæ are not uncommonly found in some flints, but they are most assuredly not always found; in some flints they are very numerous, while in others from the same spot they are exceedingly rare, and in very many wholly wanting. Now these sponge spiculæ are indestructible. The destruction of the structure of the sponge, which this theory requires as a necessary postulate, would not destroy them. How then is it that they are thus variably present? And it is important to remember that similar spiculæ are found in the chalk itself." In the first place, I deny totally that the destruction of the structure of the sponge is a necessary postulate of what the author designates as my theory. I have never under any circumstances made such an assertion, and if I had, it would have been a most egregious blunder; for the remainder of the passage I may simply say, that the conditions of the structure of the skeleton and the spicula are precisely those which every naturalist acquainted with the Spongiadæ would expect; for although the horny skeletons of the Spongiadæ are very enduring, the gelatinous interstitial substance of the sponge, which in life abounds with spicula, is exceedingly destructible, and is dissolved away from the skeleton almost immediately after the death of the animal; and in many species of *Halichondria* this is so rapidly effected, that a specimen taken fresh from the sea and placed in the hand can scarcely be retained there many minutes without its being flooded with the gelatinous matter shed by the animal, and this equally takes place if the sponge be placed in a small basin of salt water; in a few hours it will have shed the whole of its interstitial gelatinous matter, the dead skeleton only remaining. What is more natural then, than that in the silicified remains of sponges, in which the skeleton has always to a great extent been

* Page 2.

destroyed, the spicula should be but very sparingly found in the fossil, and what more to be expected than that they should be found imbedded in the surrounding chalk?

After some passing observations the author says: "If it is once admitted that flint is ever, or may be even in a single instance, found elsewhere, the theory ceases to be an explanation of the phænomena, and becomes of no value to the philosophical inquirer*." This is really so richly dogmatical that one cannot suppress a smile: does the author seriously think that he can thus fetter by a syllogism those who differ from him in opinion? But even in the face of this denunciation, I will at once admit that flint has been and is continually found elsewhere. It abounds in the mountain limestone formation of England, and I have it through the kindness of Mr. Lyell from the newest freshwater tertiary beds, from Egypt from Prof. Ansted, and out of the late Capt. Clapperton's collection from the neighbourhood of Timbuctoo, and from other parts of the world through various channels; and in all these cases it abounds in animal remains which are under the same conditions as those of the chalk flints.

The author continues, "Now, can it be shown that silex has any peculiar affinity for either the animal substance or the horny skeleton of the sponge? The contrary is known, as matter of fact, to be the case. Facts palæontological as well as recent might be cited in abundance in disproof of this necessary postulate of the sponge theory. I have undoubted sponges in my possession from the chalk, which, instead of being wholly silicified, are in part so, and in part still in the chalk, while the flint is otherwise extended beyond the boundary of the sponge." The author is evidently unacquainted with the second paper which I published in the 'Annals and Magazine of Natural History' in September and October 1842, or he could scarcely have seriously asked the question contained in the first sentence of the last quotation. If he will take the trouble to consult that paper, or to examine a few thinly sliced specimens of moss agates or green jaspers, as they are termed, from India, he will see abundant proofs of the strong predisposition of siliceous matter for the horny skeletons of the Spongiadæ. Every separate fibre which is inclosed forms a distinct nucleus, from which the chalcedonic crystals of the silex spring.

The author states, that the contrary of this predisposition is known to be the fact, but does not adduce a single proof of the correctness of this assertion, although he professes to have an abundance of such; nor does he even attempt to disprove the reasoning which I have advanced in the first paper to prove the ex-

* Page 3.

istence of an elective attraction between the siliceous matter and animal and vegetable remains; nor offer the slightest explanation of the cause of the suspension in all parts alike of the masses of siliceous spicula, the remains of polythalamous shells, small branched corals and numerous other animal bodies; nor account for the continually recurring presence of that tissue, which I have described as, and still believe to be, portions of the skeleton of the sponge to which the great mass of chalk flints owe their origin. If this description of tissue were found only in the flints of the chalk, there might remain room for doubt of its being that which I have asserted it to be; but when we find that the flints of the Portland oolite afford similar remains of a corresponding tissue, but specifically different from that of the chalk flints, and that circumstances of the same description obtain in the flints of the greensand formation and in those of the mountain limestone, such doubts cease to exist, and the fair philosophical inference is, that those tissues are in truth the remains of the spongeous bodies to which these siliceous masses have been indebted for their form.

The author then proceeds to ask*, "Where, in recent sponges, do we find the innumerable quantities of shells and other large objects that we find in the chalk flints ?" And after describing some flints with numerous shells attached to them, and specimens of which kind are by no means rare, he proceeds thus†: "I have seen, in Mr. Bowerbank's valuable collection of sponges, a specimen in which one small shell is imbedded: this may have happened in casual instances with small dead shells, but where can it be found, in recent sponges, from the most favourable spots, that they are full, as we find the flints full, of bivalves large, numerous and perfect, and apparently living when enveloped ?" In the paragraph immediately preceding the one last quoted, Mr. Smith describes his specimens of flint, not as being full, but merely as having shells attached to the surface of the flint, for he says of the shells in the conclusion to the description of the flints alluded to: "These are lying on the external surface, just sunk, as it were, in the flint, as they would sink in water, but not at all covered." But the author does not give us the slightest explanation of the extraordinary phænomenon he describes of shells partially sunk into all parts of the surface of an irregularly formed nodule of water or fluid silex "of most fantastic form," to quote his own words. It would appear most natural to suppose that the mode of their sinking in water would be at once to the bottom, and not merely to indent the surface and there remain, while, on the contrary, their position is quite natural if the

* Page 4. † Page 5.

body to which they are attached be a sponge. But where, says the author, are such specimens to be found? I answer, in my own collection, and I will show him dozens such if he will favour me with a visit to see them; and I assure him that that which he considers as so very improbable is in truth the natural habit of the Spongiadæ, which attach themselves to both living and dead shells, and in that situation they develope themselves to their full extent, freely rolling about as the tidal or other currents impel them. I have a specimen of Arca with a sponge firmly based upon one valve and loosely embracing the other, and which is many times the bulk of the shell, and the animal still remaining within the shell. Arca, Pecten, Hinnites, Ostrea, and numerous other bivalves, are frequently to be seen thus encumbered with large sponges; and I have also a large keratose sponge from Port Lincoln, Australia, which has more small univalve shells entangled in its meshes than could be counted correctly in a long summer's day; but we need not go to exotic specimens for such proofs, for if the author had only taken the precaution to have consulted Dr. Johnston's excellent 'History of British Sponges,' he would have found it quite unnecessary to have gone further to have satisfied himself of the fallacy of his own imaginations regarding the habits of the Spongiadæ, and I beg to refer him to plates 8, 5, 12, 14 and 15 of that work as pictorial proofs to the contrary of his assertion; and it is well known to every man who has paid the slightest attention to marine natural history, that *Halichondria suberea* described by Dr. Johnston, p. 139. fig. 5 and 6. pl. 12, is rarely met with, excepting partially or wholly enveloping univalve shells, and that these shells are usually inhabited by a Pagurus. I have brought up a dozen or more of such specimens at each haul of the dredge in Weymouth Bay and in the neighbourhood of Tenby, and I have many such in my possession at the present moment. It is as much the habit of the animal to be parasitic upon shells, as it is for *Dromia lator* and other species of the genus, during its life, to carry a living ambush of sponge upon its back, to secure which in its proper situation nature has stinted the growth of the two hinder pairs of legs, and directed them over the back of the animal to hook into and hold firmly the mass of sponge under which it lives and moves.

There is also another crab, which I believe belongs to the genus *Pericera*, which is in the constant habit of cherishing the growth of long fistulose sponges on the front spines of its shell, and these sponges often attain three or four times the length of the crab. I have in my possession at the present moment five specimens of the latter and ten of the former, bearing each his sponge; and in one case the mass of sponge is as big as my two fists placed together, and in several of the smaller ones the sponge is so big

in proportion to the crab as entirely to conceal it when viewed from above. There is no special preference on the part of the crabs for any particular genus or species of sponge, but these differ in almost every instance in my possession, and in one case a single crab of the latter genus has three species fixed upon its shell. The author continues: "It is assumed by this theory that the sponges grew over the shells and other organic objects which lay on the surface of the mud. But the observed facts are wholly at variance with this assumption." I have before my eyes at the moment of writing this, a sponge of the genus *Halichondria* from Van Diemen's Land of an oval form, seven inches and a half long by five and a half wide, and not more than two inches thick, which once grew spreading on the bottom of the sea, and in the under surface of which sponge there are more single valves and fragments of bivalves and univalves than I can attempt to count with success; and my friend Mr. Frederic Catherwood, whose beautiful work on the Extinct Cities of Central America has made him so favourably known to the public, informed me that during a coasting voyage in a canoe, of about 100 miles, along the shore of South America, one of his chief amusements was to lie with his head over the bows of the canoe and feast his eyes with the splendid and variegated carpet of sponges of all descriptions of form and colour, which almost covered the bottom of the shallow sea over which he was voyaging. But here again we need not transport ourselves to South America to illustrate this fact—the cave under St. Margaret's Island at Tenby will suffice for our purpose. In this place I have found seven or eight species of British sponges spreading over the surface, and rendering the rocks beautiful with their tints of green, orange, yellow, red, &c. The author then proceeds: "The Echinites alone, extensively examined, afford conclusive evidence against the sponge theory. These are very frequently indeed found in the very centre of flints. They are sometimes found with spines affixed, and therefore alive or with undecomposed soft parts when entombed. The masses of flint to which they are affixed are very frequently not attached to either of the large orifices of the shell, but to some part of the sides, while the shell is entirely filled with flint and both orifices closed. Mr. Bowerbank states that, when the shell is not entirely filled with flint, in 'the space thus unoccupied by the flint was always included one or both of the large orifices of the shell.' I do not find this fact in *any* degree borne out by my own observations." With regard to the last sentence, I can only say, the author must have been very unfortunate in his observations not to have found my assertion "in any degree" borne out; that in the cases where the shell was only partially filled with flint, one or both of the large orifices

of the shell were always included in the empty space. I can only say that I shall have much pleasure in showing him, at any time, nine such specimens in one drawer of my collection.

With regard to the fact that the shell of an Echinite should be partially or wholly filled with silex, and then attached by some part of its surface unconnected with the great orifices to a mass of flint, it is in no respect unnatural among recent sponges. Two separate individuals are often based upon the same stone or shell, and if they grow sufficiently large to touch each other, they unite organically and form one sponge; but if they be not of the same species, they will grow over or envelope each other, but never unite organically. I have several such specimens from Algoa Bay and from Wollongong near Sydney, and of the latter description I have one specimen which is composed of three species. Sometimes an individual of a different species will be developed upon the very summit of another sponge, and both live and thrive under these circumstances; such a specimen I have from Wollongong. I have also from the West Indies a Verongia seven or eight inches long so completely enveloped by a large fistulose spongia that not more than about one inch of its length is exposed, and yet both species were alive when taken from the sea. Is it unnatural then, that among the Wiltshire flints we should find one, two or three species of sponge included within another parasitical and casing sponge, and that the included ones should not be united to the enveloping one? On the contrary, it is what we see is the habit of the Spongiadæ of the present day, and therefore exactly what we should expect to have been the case with the fossil species. And again, with regard to the filling of the dead shells of Echinites and other hollow bodies with sponge. It is true I cannot show the author an Echinus shell filled with sponge, but thanks to my friend Mr. Pickering, who presented me with the specimens, I can show him nine cases of the interiors of bivalves of various genera, which have been, some wholly, while others are only partially filled by the common sponge of commerce; and what could scarcely have been expected, there is not one of the casts in which the shell has been gaping, but eight of them have had both valves closely shut, and in the ninth one very nearly so, and in this the sponge extends by means of a thin plate beyond the boundaries of the front of the valves of the shell. In these cases, which afford beautiful casts of the interior of the shell, and exhibit on their surfaces the impression of the muscular attachments and striæ of the valves, each cast has its characteristic enveloping membrane, and as the sponge has needed no support, it has not attached itself to any portion of the interior surface of the shell. The author thinks it highly improbable that teeth, wood and other extraneous bodies should be enveloped

in sponge. He can have seen but very few species of the recent Spongiadæ to be thus surprised; they abound in such extraneous matters, and moreover are the natural habitation of many species of *Balanus* and other genera of shells, just as we find among the Corallidæ certain genera and species of shells which are familiar to every conchologist as occurring in such situations and in none other. Is it then a wonder that living things should be enveloped in sponges, either ancient or modern, seeing that in many cases it is their natural and inevitable situation?

The author then alludes to the fact of the pulp-cavities of the teeth in the fragment of a jaw of Mososaurus in the possession of Mr. Charlesworth being filled with silex, and quotes this as inimical to my views of the origin of flint; and in this opinion some time since I know my friend Mr. Charlesworth shared; but after having at the British Association expressed his views regarding this interesting specimen, he with his usual liberality gave me permission to take a thin longitudinal slice from the centre of one of these flint casts of the pulp-cavity, and upon examining this through the microscope in the usual manner it was found to exhibit all the characteristic appearances of flint nodules. Two specimens of *Xanthidium* and numerous polythalamous shells were imbedded in the midst of it, and a considerable quantity of the remains of sponge tissue is apparent round the edges of the slice. Now it must be borne in mind that the fossil was but the fragment of a jaw when imbedded in the chalk, and I believe, from my recollection of the specimen, that at the time of its imbedment it had lost the lower edge or keel of the bone: is it therefore to surprise us, that such bodies as sponge gemmules, often much less than one hundredth of an inch in diameter, having ciliary locomotive power, should insinuate themselves into the pulp-cavities, either through the nerve or blood channels, or by means of the space between the tooth and its socket after the animal matter lining the latter has been removed by maceration, and there develope themselves and fill up the space of the cavity? Neither is it unnatural, that such minute living bodies as microscopic foraminifera and *Xanthidia* should be found in such a situation, as sponges are continually inhaling currents of water through one set of canals and ejecting it as continually through others, and this with no small degree of power.

When I was at Tenby some years since, I placed some specimens of *Halichondria panicea,* Johnston, in a shallow dish of sea-water, and in one of them in which the orifice of the excurrent canal was more than half an inch below the surface of the water, the outpouring current was so strong, that when the reflection of one of the bars of the window was brought over the orifice of the sponge, the reflected line was curved to a very consi-

derable degree, so as to render it evident that the surface of the water was elevated by its power the tenth or the eighth of an inch, and some light dust shaken over the spot was dispersed in a circle with great rapidity. I have seen the same phænomenon in vivid action with *Grantia botryoides* in a closed cell filled with sea-water beneath the microscope, when at Weymouth in the year 1845. Need we wonder then, with such powers inherent in the Spongiadæ, that minute animal or vegetable organisms should be found in such positions as those alluded to by Mr. Smith? But there is yet another way in which the filling of the pulp-cavity and the space intervening between the tooth and its socket may be accounted for in the small jaw figured by the author, and it is simply this: that as the whole of the fragment of the jaw has been built over by the sponge originating the flint, it is quite natural that it should have insinuated its fibres into those spaces in thin plates, and such thin plates of single layers of reticulated fibre, not exceeding the five-hundredth of an inch in diameter, may be frequently seen by the aid of a lens in the sponges of commerce, especially at the termination of the excurrent canals of the West Indian species. But in reality Mr. Smith's specimen needs none of these conditions to account for the presence of the silex in any one part of it more than in another, as the whole substance of the jaw is more or less silicified, which fact was not observed by the author at the time of the publication of his paper. There is nothing more surprising in this replacement of carbonate or phosphate of lime in bone by silex, than there is in like replacements in the shells of the greensand formation and of the London clay, *Voluta luctator* and other shells. The same phænomenon takes place in the corals of the mountain limestone of Derbyshire, Ireland and elsewhere; and this I believe to take place without the presence of any degree of heat above the ordinary mean temperature of the earth, and for this reason; that in almost every flint that I have examined, I have found evidence of chalcedonic crystallization wherever there has been a small space originally not occupied by spongeous substance. And in almost every moss agate it may be seen that the fibres are the prevailing nuclei of crystallization; from these they constantly radiate until the various crops of crystals meet at their apices and form ultimately the solid mass of the agate. In fact, the process of siliceous deposit in these organized fossils appears to be precisely the same in principle as in the deposit of siliceous matter in hollow spaces in rocks of igneous origin, only that in the first case the place of crystallization is determined by the presence of the organic fibre of the sponge, and in the latter case simply by the sides of the cavities in the rock. We find also in chalk flints, that where there has happened to be a large central cavity, the sides are often coated

with half an inch in thickness or more of pure chalcedony, and then succeeds a crop of regular crystals of quartz. The like is familiar to every mineralogist in agates from rocks of volcanic origin, in the cavities of which, the water, perhaps containing but a very few per cent. of silex, may by gradual and continuous filtration have deposited the silex long after the rocks had ceased to possess a greater degree of heat than the ordinary temperature of the earth.

If, on the contrary, we imagine a high degree of thermal heat necessary for the conveyance and deposit of the silex, how is it that the water at this high temperature spares the carbonate of lime in the beautiful and delicate shells which are often attached to the surface of the chalk flints, and the numerous remains of cartilaginous and other fishes, crustacea, and other delicate animal remains which abound in a most perfect condition amidst the very flints that are supposed to require so great a degree of thermal heat for their formation? And if the deposit of the silex be determined by any great degree of thermal heat, it may naturally be supposed that it would be deposited somewhat in the form of that precipitated from the waters of the Great Geyser and other such springs; but this is not the case. In the flints and agates the normal form of the deposit is the compressed acicular crystallization of chalcedony; while in the latter it is purely amorphous, the highest power of the microscope affording not the slightest indications of crystallization: in fact, it is the well-known mineral, siliceous sinter. I have examined specimens of this mineral bearing the impressions of leaves recently brought from the Great Geyser by Mr. C. C. Babington, and with a power of 500 linear it presents a purely resinous or glassy structure; not the slightest trace of radiating crystallization even from the parts which bear the impress of the leaves.

The author then treats of the fossilization of Choanites and Ventriculites, and describes them as imbedded in flint, and possessing "a light floating elegance of form as if still enjoying life in their native liquid element; and which facts assure us that they were thus suddenly and instantaneously fixed in a moment of the highest vitality." I really cannot understand how the author arrives at this conclusion, that because they retain their form they were necessarily imbedded alive. We are all familiar with the very long time that a piece of common sponge will do duty in a water-filter, for months or even years, without the destruction of its texture; and the recent genera to which the fossil sponges termed Ventriculites and Choanites belong, are of a much stronger and more horny structure. The recent type of the former I have received from my friend Capt. Ince, R.N., who procured it at Torres Straits, and another species from the Phi-

lippine Islands by Mr. Cuming; and I have the fac-simile of *Choanites Kœnigii* both single and double, and of about the same size as the fossil species from Wollongong near Sydney, Australia.

The author, dismissing the evidence to be derived from the internal conditions of flints, then proceeds to consider their external forms, and says: "And we shall find, on taking a careful review of some facts of the external forms and modes in which the flints are found, that the sponge theory is not only wholly unsatisfactory, but absolutely impossible." I reply to this simply by asking the author why such an origin for the chalk flints should be impossible, seeing that the author does not deny the existence of other sponges of undoubted character in a silicified state; and but a few paragraphs previous to the one quoted, he describes the investment of Ventriculites by silex as if it were quite a natural event. Now if one sponge may be thus invested and imbedded by silex, why not another? To me, the whole difference appears to be, that the one was more prone to decomposition after death than the other, and therefore that we find its skeleton in a worse state of preservation than in fossilized Ventriculites and Choanites.

The author having obliged me with an inspection of the specimens he has figured, I may briefly say, that the supposed revolving particles in flint represented by the woodcut, page 11, and for the peculiar motion of which amidst the imaginary fluid flint, the author offers no principle, are in my idea merely the remains of one of the large internal canals of the sponge, the natural arrangement of the particles of which, as a matter of course, presents the appearance described by the author.

There appears to me nothing in either of the originals of figures 2 and 3, Plate I., that is in any degree anomalous. In No. 2 a portion of the stem (*c*) has broken away from the base of the Ventriculite after it had become silicified, an accident very likely to happen during the subsequent elevation of the chalk, and which process was probably going on during the period of its deposit. No. 3 presents the imbedment of fragments, some of which appear to have been shells, on the under surface of the flint, the carbonate of lime having been replaced by silex; others are simply fragments of older flints. Figure 1 represents a mass of flint which exhibits an appearance of having been deposited in concentric layers which are exposed by what seems to have been an irregular decomposition of its surface. I have often met with this anomalous structure, containing the same organic remains as those in the common chalk flints, but I have not yet obtained a clue to the origin of its peculiar form: nor do I think Mr. Smith's hypothesis of two currents in contrary directions, and one whirlpool in about six superficial inches, at all likely to afford that clue, as unluckily there are another set of contrary currents

in opposition to the figured side also to be explained, and moreover the author does not give us any principle upon which these minute currents can be accounted for.

In treating of the probable origin of these figured specimens the author says *: "The movement which caused this fracture and impelled the pieces on to the yet fluid mass was probably the same which caused the whole surface on which the fractured pieces alighted to slip forwards, and which surface and the mass beneath it, probably by the very agitation thus caused, instantly solidified, leaving the ridge *a b*, and fixing the fractured pieces firm.

"This case illustrates and demonstrates all the conditions already noticed; extreme liquidity and rapid solidification of the flint, together with the soft state of the surrounding chalk."

Here the author distinctly recognises the theory of the gelatinous condition of flint, although he appears in the commencement of the paper to have repudiated it, and in the following page he says †: "Where organic remains of any considerable size, or grouped in particular masses, happened to be abundant and lie near one another, they acted as separate centres, while the solution was attracted to them in a mass." I must confess that this mode of accounting for the fantastically-formed nodules of flint is perfectly incomprehensible to me. I cannot by any stretch of the imagination conceive a mass of saturated solution of "extreme liquidity" preserving its integrity for a moment at the bottom of the ocean, and especially amidst so many minute currents as the author supposes to exist. But let us see what foundation we have for the supposed "masses" of solution of silex.

Throughout the whole of the report of Dr. Turner's lecture, there is nothing touching the existence in nature of a gelatinous condition of silex beyond the supposition of Brongniart that such might be the case, and which is expressed thus ‡: "In the formation of chalcedony and flint, it was most likely, as Brongniart supposed, that the silica, as in operations in the laboratory, was deposited in a gelatinous form, hardening gradually by evaporation and the cohesive attraction of its particles." By the use of the word *evaporation* it is evident that the passage is not applicable to the conditions of silex in solution in the depths of the ocean. The author then proceeds thus: "The regularly disposed lines which were so beautifully displayed in some varieties of chalcedony, seemed owing to successive deposition—one layer succeeding another, each assuming the form and irregularities of the preceding,

* Page 15. † Page 16.

‡ London and Edinb. Phil. Mag. vol. iii. p. 27.

and differing in tint according to the absence or presence of small varying quantities of foreign matter, such as iron or manganese."

It is evident from this passage that Dr. Turner had chalcedony more especially in his mind when he penned this passage, and that in reality he believed flints to have originated from organized bodies by slow infiltration, for the last passage quoted is immediately succeeded by the following one: "In the case of flint it was necessary," he said, "to account for that remarkable tendency which silica possessed to occupy the place of organic matter, as exemplified by the specimens of flint, silicified wood, and coral on the lecture-table. This phænomenon the lecturer thought might be explained on the principles which had been developed that evening. Siliceous solutions infiltrating through organic masses in progress of decay, might readily be decomposed by the affinity of gases or other compounds generated during slow putrefaction, either for the silica itself or for its solvent. In either case a deposit of silex would result." From this passage it would appear that, although he quotes the supposition of Brongniart, he was for his own part of opinion that the slow infiltration and deposit through the agency of the decomposition of organic bodies had been the means of the formation of flint.

Let us now inquire what foundation there is for the supposition that flints are formed from detached masses of gelatinous solution of silex. Have such solutions ever been found in nature? Is there a single writer on chemistry or mineralogy who describes such a condition of silex as natural? Mackenzie in his account of Iceland speaks of the vast deposits of silex in the form of siliceous sinter, and of its encrusting the living stems of grasses, but says not a word of its ever occurring in the gelatinous form under any circumstances; and Mr. C. C. Babington, who has recently returned from a visit to the Geysers, confirms this account, and also has told me that in no case did he see anything in the form of a soft or gelatinous deposit of silex in the neighbourhood of the springs, although he saw the Great Geyser more than once in full action. Certainly if there be any place in the world where we should expect to find this gelatinous form of silex deposited in a natural condition, it is there, where the waters are so high in temperature and so abundantly charged with the earth in solution, and yet nothing approaching to it has ever been observed by the most enlightened and observant visitors of the spot. From our own knowledge of nature, therefore, we may reasonably arrive at the conclusion that such a natural condition of silex as the gelatinous one is no more to be expected than that we should find pure potassium or sodium occurring in the bowels of the earth, or any other such substance which is the

result of chemical science only, and not the natural condition o the bodies in question. We can only therefore regard the idea of Brongniart as a pure hypothesis, which is very much easier to invent than it is to carefully work out the truth by laborious investigation.

And again, let me ask, what is the necessity for resorting to far-fetched hypotheses to account for the presence of the silex, when we have such frequent and obvious evidences of its great prevalence in solution in water under almost every description of circumstances? We have but to examine wheaten straw to be assured of its having been imbibed by the plant from the water of the soil during its growth and secreted as one of its component elements in great abundance; and every little boggy hole that is filled with water, every pond, ditch, lake, river or sea, swarms with Desmidiæ and infusorial animalcules alike secreting silex as their outward covering and protection, evincing in all these situations the abundance of the earth in question in solution; and geology is proving to us daily that such also has been the case from time immemorial. No vast pressure, no high temperature is in reality required to sustain silex in solution, and this is readily to be proved by reference to springs in our own country, as at Bath, where the hot-bath spring yields 128 gallons of water per minute, or 184,320 gallons per day; and as each pint of the water, according to the analysis of Mr. R. Phillips, contains one-fifth of a grain of silex, there is consequently $35\frac{1}{2}$ pounds of solid silex poured forth in solution in every day's discharge, or 12,857 lbs. per annum, and the water has a temperature of only 117° Fahr. The Great Geyser in Iceland jets forth a column 200 feet high and 10 feet in diameter at a boiling temperature, and contains, it is said, 31·38 grains of silex per gallon. If these two comparatively insignificant sources produce thus much of silex, ought we to be at all either surprised or astonished at the universal presence of this earth in solution?

PROCEEDINGS OF LEARNED SOCIETIES.

ZOOLOGICAL SOCIETY.

December 8, 1846.—George Gulliver, Esq., F.R.S., in the Chair.

A paper was read containing descriptions of 38 new species of Land-shells, in the collection of Hugh Cuming, Esq., by Dr. L. Pfeiffer:—

1. PARMACELLA CUMINGI, Pfr. *Parm. testâ depresso-semiovatâ, tenuissimâ, striatâ, lineis spiralibus subtiliter decussatâ, diaphanâ,*